THE METHOD OF SCIENCE AND THE PUBLIC SCHOOL

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"God forbid," says Bacon, "that we should give out a dream of our imagination for a pattern of the world"; a judicious and earnest prayer, an affirmative answer to which has often been denied to us, for patterns of the world which are such stuff as dreams are made of are still not infrequently turned out from the loom of man's imagination. Bacon, as all know, did not content himself with empty petitions, but made a famous effort to develop and establish a method of investigation of the truth of nature which should insure a real and not an imaginary product. His "inductive method" brought him immortal fame, but it has, nevertheless, in the form in which he left it, been of remarkably little use to the actual investigator. His idea that general knowledge of the highest character could be acquired from immense accumulations of facts by mechanically repeating on them the process of inductive generalization till laws of the highest grade were reached, has proven unfruitful of scientific results, and, indeed, impracticable except in limited fields; but it has nevertheless contributed largely to the formation of that finished method of modern science a discussion of which I have been asked to open.

The term, method of science, is often somewhat loosely used, and is not commonly applied, I think, to the abstract sciences of logic and mathematics. The method of physical science is what is ordinarily meant by it, and we may provisionally use it in this sense; for, as I shall presently try to show, the full physical science method is the complete method of science, other scientific methods being fragments or abbreviations of it only. But the physical sciences—chemistry, physics, astronomy, geology,

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biology, etc.—do not agree in their methods in all respects. The method of chemistry is so different in some respects from that of physics that one trained in chemical methods needs much more than a mere knowledge of the facts of physics to become a physicist, and no training in physics with chemical knowledge superadded will make an expert chemist. A man may be never so much a chemist or physicist or both, but he cannot then become a competent biologist by merely learning any number of facts about biology; he must still have his training in the special biological method. But notwithstanding minor diversities in the methods of the separate sciences, there are certain main features common to them all which may be abstracted, generalized, and stated in comprehensive form; and these constitute what we may call the method of science in our sense of the term.

But what shall we mean by method in this discussion? Not the mere use of tools of any sort, however complicated and invaluable; not the manipulation of apparatus, or any form of mechanical operation on anything. Tools, apparatus, and laboratory manipulations and experiments are helps to observation, indispensable often in the accumulation of facts, but they do not themselves accumulate facts, and they do not in the least help to organize the facts accumulated, or to reason on them when organized. The method of even physical science is indeed a mental method, and the study of this method is a study of the action of the scientific mind while engaged in the pursuit of scientific truth. The subject is thus not physical but psychological, and the question which we wish to find an answer for is, I think, substantially this: What are the general features of mental method common to all sound and successful investigations in the physical, or concrete, sciences? This is a question of the greatest importance to us all, for this method of science must always be our final means of defense against the ravages of unbridled fantasy in the field of general truth; a defense never more needed than now by that mass of the ignorant, the partly educated, and the poorly educated, who still make up the great bulk of humankind. I shall try to answer this question as well as I can in a little time by giving an outline description, brief and necessarily somewhat crude, of the various steps or stages in the method of the scientific man engaged in the serious study of a new, difficult, and complicated problem.

The first step in any such study is the investigation of the investigator by himself. He is to look on himself as his own apparatus of research, certainly defective in various ways and never capable of being fully perfected for its purposes, to be studied, therefore, with a view to correcting its action where possible, and of guarding against its deficiencies and allowing for its irregularities where this cannot be done. What is the temperamental bias of the investigator? Is he unduly optimistic, or is he too easily discouraged? Is he already committed to general views which are likely to create a prejudice in the case which, as the lawyers say, it will take evidence to overcome? Is he too quick to generalize and thus likely to proceed under the influence of premature opinions; or is he overcautious, hesitating to draw conclusions which are warranted by the evidence, and thus likely to stand in his own way and block his own advance? Has he a well fixed habit of sure and steady work, such that he may safely close each stage of his investigation as he finishes it; or must he go over each step of it again and again, suspecting his own thoroughness and exactness, till everything has been tested in every way he can contrive? These are examples merely of the searching questions which the conscientious investigator puts to himself till he is sure that he understands himself thoroughly as an apparatus of investigation. And he cannot finish this task of scrutiny, discipline, and self-correction once for all, settling down thereafter satisfied. He is fortunately organized indeed if he does not have to keep a close eye on himself for a very long time, as one liable to relapse into the inaccuracies of the ordinary untrained man, or to fall incautiously under the influence of original defects not yet wholly overcome.

His systematic research he will begin by an accumulation of the facts necessary to a study of his problem,—if these have not been already accumulated by himself or by some one else,—and he will be specially watchful at this point that no superfluous assumptions slip unnoticed into the company of his primary data; no assumptions, that is, which are not necessary to the beginning and the continuance of his work. There is no error in pseudo-science more common than this perfectly fatal one of a conscious or unconscious assumption in the beginning, of things not known and not in the nature of the fundamental presuppositions of scientific thought. Assumptions we all of us must make every time we rationally think and every time we deliberately act, but it is a fundamental principle of scientific law that the investigator shall make no assumptions not necessary to the constitution of his science and to the use of its legitimate method. If a physicist, he will not call in question the real existence of energy or motion or the validity of the mathematical laws which he makes use of in his reasoning; if a biologist, he will assume the soundness of the generally received conclusions of physics and of chemistry; and, whatever his specialty may be, if a scientific man he will not as such question the real existence of an objective world, the truth of the law of causation, or the principle of the uniformity of nature. These are assumptions so necessary to his purpose that if he could work without them at all it would only be at an enormous expense of labor and convenience. He may be perhaps a thorough-going idealist in metaphysics, but when he steps into his laboratory he leaves all that behind him, for there he thinks in other terms, and his mind speaks a language which he could translate into the dialect of idealism only by using the dictionary on every word. And in the accumulation of his facts, he will see—as well as he can at this stage of his procedure—that each is well and thoroughly known; that all are pertinent to his end; and that they are sufficient in number, variety, range, and bearing to furnish a strong and broad foundation for the superstructure he has planned. This is the period at which great waste of labor may easily occur through the gathering together of materials excessive in some places, deficient in others, and wholly useless, because inapplicable, in others still. The end in view, the question to be answered, must be carefully kept in mind as a guide to choice.

Then, as he goes on, after this accumulation stage is passed, he classifies his more or less complex material, he assorts his facts in bundles of like kinds, putting a general label on each one, and goes on perhaps to make larger bundles of these bundles, and then larger bundles still of these, so labeling each package as he goes that he can thereafter use the general label as a substitute or symbol for the compound package itself. And this classifying and generalizing process is also guided by the end he has in view, and the generalizations reached by it will all be such as have a bearing on the subject of his inquiry. If he does not thus directly reach the general truth of which he is in search—and in a difficult subject he commonly will not—he will at least greatly limit the field of his inquiry. Among his several generalizations may be one or more which will seem to point the way to the true law or principle which he seeks. And then he begins to guess—within limits; he turns his imagination loose—under guard; he invents hypotheses—consistent with the facts; he employs the method of multiple hypotheses, perhaps; that is, he imagines all the various theories he can think of which are not contradicted by the facts as they then appear to him, and somewhere among the fish now tumbling into his net he expects to find the one with the golden coin in its mouth.

So far we have followed the accumulating, the combining, the inductive side of the method of science, but now, having reached our higher generalizations or our tentative hypotheses, one or more, how can we choose with certainty between them; how may we know which of them is true, or whether any one is true? We must proceed somewhat as the pupil does at school when he proves his division by multiplying divisor and quotient to get the dividend; that is, we must reverse the process, and having reached certain general conclusions by induction from particulars we must now reason deductively from these general conclusions to particulars again, and then must compare our reasoned-out particulars with those derived from observation or experiment. We do this not only once but again and again, in as many ways as we can think of, and if all turns out as it should by our hypothesis, then we are sure that our problem is correctly solved. If we are dealing with several mutually inconsistent hypotheses instead of one, then we reason out the various consequences of each, compare all with the facts previously known or subsequently ascertained, and exclude those hypotheses which are not sustained by this method of trial, this verification process as it is commonly called.

The accumulation of pertinent observations, the classification and generalization of them, the framing of hypotheses from the materials thus obtained, deduction from these hypotheses and comparison of the products of these deductions in every way possible with new facts till substantial certainty is reached, these are the general steps of the method of physical science. It is time to say, however, that in practice, and especially in some of the sciences, this whole round is rarely followed out in full. Short cuts across corners, abbreviations or even omissions of certain steps of the process are often possible to the expert, who may see, as by a flash of judgment, whither an investigation is tending, and so jump to the point at once; but even an expert may not dispense with a sure starting point and a rigid verification. If one omits or obscures these, we may know that he is not even a scientific man at all.

In physics or in chemistry a single observation or experiment is often enough to suggest to an acute and fertile mind a hypothetic explanation which brings the experimenter at once to the verification stage of his inquiry. In many departments of science vast masses of material have already been accumulated, classified, and generalized in advance, ready for the use of any one; and investigation in these departments may begin with imagined hypotheses, followed by verification through experiment and by added observation. In mathematics especially induction was long ago practically completed, and the mathematician is occupied now only with deductive and verification processes. Physics and chemistry also have gone some distance on the same road, and general laws have been established in considerable number and of extensive scope, from which deductions may be made at once, and by reference to which new facts may be explained without the tedious preliminaries of extensive observation and repeated generalization. In the vast field of biology, on the other hand, full as it is of the most perplexing complications, few stable generalizations have as yet been reached, and there most students are still busy with the inductive side of the operation. They are working towards general propositions, while the mathematicians and physicists are working from them. Induction predominates, in short, in the more complicated, that is the less developed, sciences, and the deductive method in those which are far advanced.

The appreciation of these differences of method in the various related sciences is of great practical importance, since it is not an uncommon error to apply the method of one science in the field of another to which it is not appropriate. One trained mainly in chemistry, accustomed to infer with certainty the characters of a whole class from the results of an examination of his first example of it, knows little of the tedious repetitions of observation on multitudes of individuals and the complicated processes of generalization necessary to establish class characters in zoölogy or botany; and the mathematician, accustomed to go at once to his general principles as an unalterable point of departure, can scarcely appreciate the requirements of an investigator who must start from individual instances, with general principles as the halfway house to his goal.

I must next make brief reference to two kinds of operation which, though they may not properly be described as parts or even varieties of the strict method of science, are, nevertheless, so helpful in scientific research, of such constant utility for inductive investigation, that the practical investigator would spare almost any other tools from his workshop as willingly. These are reasoning by analogy and the calculation of averages, the latter often used as a basis for the estimation of probabilities also. Analogic reasoning and the estimation of probabilities are indeed such clever tools and so convenient to the hand, they work so easily and so rapidly, that one is often strongly tempted to pick them up when only a heavier instrument and a slower operation are really adequate to the task in hand. On this account they commend themselves especially to the unscientific as a substitute for the scientific method. The principle of analogy, "like causes produce like effects," has a very plausible sound, and if it were always and strictly true it would save an immense amount of minute comparison and critical analysis of things which are clearly much alike, but not certainly like enough or like in the right way. What the untrained man will accept as a conclusion and proceed to act on the trained man will use as a promising hypothesis merely, not to be fully accepted till verified. But in the suggestion of hypotheses the method of analogy has a brilliant record in the annals of science in every age.

Scarcely less useful and scarcely less treacherous is the method of averages and the estimation of probabilities. From an imperfect examination of a part to assume a sufficient knowledge of the whole, is one of the forms of the abuse of a method which, properly used, enables the student to penetrate to definite conclusions through thickets of difficulty which would otherwise be wholly impassable to him.

I am not at all sure that my abstract description of the process of the scientific search for truth has been sufficiently clear to all of you to permit me to dispense with concrete illustration, and it may prevent misunderstandings and remove ambiguities if I illustrate it by some two or three examples. Let us take first a plain and simple problem, to the solution of which the full round of the scientific method may profitably be applied. I should like particularly to take an illustration from the work of one of the agricultural experiment stations because these are excellent examples of organizations for strict scientific research, the thorough method of whose work is too little appreciated, and the educational and scientific value of whose results is too little known.

Let us suppose that an experiment station assistant is charged with an investigation intended to lead to a discovery of the best method of feeding stock for the purpose of growing them rapidly and fattening them early with the greatest economic profit. He might conceivably begin in any one of several ways, but he will most likely first avail himself of the various conclusions of other men, scientific and practical, who have had experience or made investigations in his field, adopting their announced results as his hypotheses merely, and proceeding at once to test them by deduction and experiment. That is, he will assume that the necessary accumulation and generalization of data have already been made by other men, and that he can omit these first steps of his research. Suppose, however, that he lacks confidence in previous work, and feels it necessary to begin at the very beginning for himself Under these conditions he would probably first search out and bring together the largest possible number of instances of marked success in cattle feeding, with full particulars of the conditions and procedure in each. For comparison with these he would collect a large, if not an equal, number of unsuccessful instances. Each set of cases he would then compare among themselves, the first with a view to ascertaining what were the common features of the food and of the treatment generally in the practice of the successful feeder; the second to see also what common features could be distinguished as characteristic of unsuccessful practice, but especially to see whether those found characteristic of the successful group were wanting in the unsuccessful. In this manner he would analyze his data by what is known to inductive logicians as the joint method of agreement and difference. Remembering that a multitude of factors would affect results besides the kind of food made use of, that some of these factors would be favorable and others unfavorable in each group of cases, and that the effects of feeding would thus be more or less obscured, he would not stint himself in respect to instances, for the larger his accumulation the more completely these obscuring tendencies would counteract each other, leaving the different effects of different food to stand out clear and unmistakable. He would apply the methods of averages, in short.

The general propositions thus arrived at as to the practice of successful feeders would at least contain materials for hypotheses concerning the most successful feeding practice possible, and these hypotheses our experimental agriculturist would next proceed to frame, and, with them in mind, he would arrange his scheme of experimentation to test them separately. And, knowing, as he will, that in the rearing of every animal, no matter how carefully chosen and skillfully handled, peculiarities of constitution, of condition, and of treatment are sure to occur such that no safe inductions can be made from single instances, he will multiply identical experiments and eliminate his variations and errors by averaging his results. Remembering also the inevitable differences of environment and condition under which stock is reared in his state, he will so vary his feeding experiments as to imitate as closely as may be these interfering circumstances, and, finally, by a comparison of these results, after some years of careful work, he will reach conclusions as to the ideally best food and the best methods of feeding for various kinds of stock under the various conditions of actual practice on the ordinary farm.

In further illustration of the scientific method as practiced in the experiment station, I will describe not an imaginary but a real research, which is of special interest to us here from the fact that it was conducted partly at Cornell University and partly at the University of Illinois, and that its very notable results have been published in part by both institutions.

In 1896 Dr. Cyril G. Hopkins, then Experiment Station Chemist in Illinois, now Professor of Agronomy in the university of that state, set himself the task of ascertaining whether Indian corn might be improved for human use in respect to its chemical composition; whether breeds or varieties of corn might be developed which should contain more proteids than the present average, or more fat, or less starch, for example. In asking himself this question the investigator framed, in effect, an affirmative hypothesis. He assumed for the purposes of his investigation that such chemical varieties might be developed in corn, and then he undertook to test or verify this assumption by deducing its consequences and by comparing them with the facts. We have first to notice that this hypothesis was suggested to him by analogy. "That the chemical composition of corn can be changed," he says, "seems reasonably probable from the changes which have been produced in some other plants—notably in the sugar beet." And again, "the method of procedure which seemed most promising is based upon the common method of making improvement in animals, namely, selecting the best examples of the desired type and breeding successively and under the best conditions from that stock, retaining from each generation only the highest types obtained. This is practically the method by which the sugar content of certain varieties of beets has been increased from less than 5 per cent. to 12 per cent. or even 16 per cent."

Now, to the formation of a variety by repeated selective breeding two things are indispensable as preliminaries; the organism under experiment must be variable, and its variations must be capable of transmission by inheritance. If from corn new chemical varieties can be formed, then corn must vary in chemical composition, and the chemical peculiarities of a given selected lot must tend to reappear in its produce. Deductions, these are, from the assumed hypothesis, to be compared with the facts as ascertained. Is the chemical composition of corn variable in definite ways? A long series of analyses furnished the grounds for a generalization in the affirmative. Different ears of corn do vary widely in the proportions of their main ingredients, and these chemical variations appear within the same variety, within the crop of the same year, within the product of the same small plot. Are these variations of chemical proportion reproduced at all in the next generation of the plant? Many series of plantings from variously selected seed, carried on now for four successive years, have furnished the materials for another affirmative generalization, to the effect that the chemical peculiarities of the seed do tend to appear to a notable degree in the produce of the planting. The original hypothesis is thus far verified, and the foundation is laid for the next step in the investigation, which is to ascertain whether *permanent* varieties can thus be formed which will perpetuate themselves indefinitely without an annual selection of seed by the use of special tests. To answer this question finally, to test this hypothesis thoroughly, will take additional years of expert and faithful labor.

Allow me a further instance or two from a field in which I am personally more at home—that of the Illinois Biological Station in operation under my direction. In August, 1898, I detailed a young assistant, Wallace Craig, for a study of the local distribution and the movements of the fishes of our station field, with a view to making out the choices of environment, the preferences as to situation of the various species of fish under varying conditions and at different times of the year, together with any similar matters which might come to our net. Guided by analogy, I undertook to transfer and adapt to ichthyology the statistical method of research, the method of averages that is, which has brought an abundant harvest of new knowledge to the student of the forms and multitudes of minute aquatic life. Many fish traps and nets of uniform character were kept continuously set for eight months—from August to April—or were used at regular intervals in carefully chosen situations. The product of each net and trap was determined and counted for each species every few days, and later the data as to species of fish and the relative numbers of each were tabulated for each situation and each date. These collections and observations were the primary data of our investigation, so grouped and classified in the tables as to disclose the general conclusions of which we were in search. By a comparison of totals and averages for the various situations in our field it was learned, among other things, that the fishes of the locality are divisible into three main groups, inhabiting respectively the rivers, the lakes, and the creeks. These groups are distinguished not by differences with respect to the presence or absence of species but by differences in their relative abundance, and they are clearly recognizable even where there is a free open-water connection between their habitats. The river fishes are in general the oldest and the least specialized and the most closely related to those of the Mississippi, into which indeed all of them still range, a few migrating from the gulf. The fishes from the bottom-land lakes are, on the whole, more recent forms than those of the river, smaller, and more highly specialized. Those of the creeks are smaller still and most highly specialized of all. Further, the Illinois River at the situation studied is frequently muddy on one side and clear on the other owing to differences in the banks and in the character of the entering streams. Where this difference exists clear water and muddy water groups of fishes are distinguishable, but when for any reason this physical difference is obliterated for a time the distinction of these groups is obliterated also. The spring migration impulse connected with the search for breeding grounds likewise confuses all these groups for the time being in a general movement up the streams and into shallow water. Each of these generalizations, I need hardly say, invites to additional research as to its general and uniform validity and as to the causes of the phenomena which it sets forth.

The preponderance of observation, classification, and generalization in biological work over deduction and experiment is illustrated by the foregoing, but better still by the general operations of our Biological Station during the past six years. Founded in 1894 especially for the experimental investigation of œcological subjects, this Station has now published several hundred pages of contributions to knowledge and has some hundreds more just passing through the press, but has not yet reported or matured so much as one experiment. This is because in such a field one cannot even see the outlines of the special problems to be worked on till its contents have been surveyed, classified, and analyzed, and this preliminary procedure is with us scarcely yet complete.

I think that I need not further multiply instances of the application or the variations of the method of science, and I have accomplished my present purpose thus far, if I have given you an outline of its main features comprehensible to those not already familiar with the subject, and have opened the way for a discussion of the relations of the scientific method to the teaching of the sciences in our public schools.

At this point I especially feel the necessity of proceeding carefully, since my opportunities to learn by personal observation or by authentic report what is actually done in the science work of the schools are much inferior to those of many here present. On this account I will limit my comments to certain methods of science teaching, wherever used, considered in relation to the method of science as above described. Take, for example, the laboratory method in biology, commonly so called. The student under instruction by this method sits at his table with a lifeless object before him of more or less complicated structure, and a book beside him which is essentially a manual of directions as to the mechanical routine of his work, a nomenclator of the parts of the object under his examination, and a more or less definite description of it, with broad intimations and pointed hints as to features not specifically described which it is desirable that he should see. He reads the book, he does the things he is told to do, he looks at the things he is told to see, he observes and records and draws, and he listens to the remarks of his instructor, and in it all he does not so much as lift one foot from the earth on which rests the lower end of the ladder which we call the scientific method. From our standpoint this is all prescribed and directed observation merely. If in passing from object to object of the series laid out for him he makes comparison of one with another, noting resemblances and differences between them, then he does lift his foot as if to place it on the lowest round, but he can hardly be said actually to place it there unless he goes at least so far as to frame a definition of a class by combining the results of these comparisons into a list of characters common to the group. Even the determination or identification method, if we may so call it, despised and rejected of teachers of biology for these many years, carries the student at least as far as this in the practice of the scientific method, and gives him at the same time a more exacting elementary drill; for one cannot determine a plant or an insect species without close and careful observation or without answering positively for himself, yes or no, to a considerable series of questions, each compelling the comparison of his visible object with a conceptual image.

If we turn to what I suppose to be the more common methods of instruction in the chemical and physical laboratories of the public schools, we find no very unlike condition of things so far as I now see. A chemical determination calls for a very different mechanical procedure from that of a botanical or an entomological one, but the mental act is almost precisely the same. The student observes his mixture of substances and compares its behavior or the product of its reactions with certain descriptive matter in his book or in his memory and makes an identification based on that comparison, and with this elementary classification process his use of the method of science commonly seems to stop. In the physical laboratory his work is either illustrative of general principles taught him, or, at the best, he works out what is called a problem: given such and such an apparatus to prove by experiment such and such a law. His procedure is practically dictated, and his result is predetermined. If he reaches a different conclusion by his experiment from that prescribed to him in advance, which is it that he doubts, the experiment or the law? Unquestionably the former, and he works it over and over again, if necessary, till the answer to it "agrees with that in the book." This seems to be largely a drill in a mechanical operation rather than a practice in a mental method.

We must distinguish between a study of science and scientific study, between instruction in science and scientific instruction. If by scientific we mean "pursuant to the method of science," then the courses just described are unscientific, and instruction in them is instruction in science, it may be, but not scientific instruction. If we would teach science for its method, we must so teach it as to bring that method into play; and this must be our main object and not a secondary or supplemental one. This means, as it seems to me, that we must supply the materials and aids and favoring conditions for untrammeled observation, for independent induction, and for experimental verification, and that we must furnish suggestive models of the method of research. These things we must do with at least as much care, ingenuity, and precision as have been displayed in our efforts to transplant into the high school and to adapt to its work the methods, equipment, and ideals of the university laboratory of morphological research. I do not in the least doubt that by thus making possible some knowledge of this mental method and some general exercise of its application we shall be doing a great service to education and the state. For the method of science is not to be understood as a method for the scientific merely; it is to be taken as a general method of certainty. It is the only means of certain conclusion over much the larger part of knowledge and experience. Even when the conclusions reached by it are themselves uncertain, it helps us to a certain knowledge of the degree of that uncertainty. It is the method of valid results in the objective world, of secure foundation for thought, of certain warrant for sustained and complicated action To the practical man it is mainly useful in ordinary life as a measure or standard of certainty. He can rarely wait for its full and formal application to the small affairs of business, but must approximate its decisions as well as he can and take his risk of error as in the end the most profitable course, but it is extremely useful to him to have some means of judging what that risk may be; and in great affairs of business or of state, where expense of time, of labor, and of thought are justified by the gravity or the permanence of the interests involved, then it is the prime safeguard of enterprise, the promoter of welfare, a powerful instrument of progress. I think of it often as a simple but mighty engine composed of a few great bars and bolts, variously changeable in form and in articulation, wonderfully adaptable to widely different purposes, fit to point a needle or to pound out a walking beam, by whose sole and sufficient aid the great builders of the past have reared the permanent framework of our civilization. To have one's hand on the lever of it with the knowledge of its mode of action and its powers is to be a modern man, equipped for modern life. To be ignorant of it or indifferent to it or contemptuous of it is to be exposed, helpless and unconscious of the need of help, to innumerable follies, quackeries, and superstitions, ruinous to oneself and dangerous to society. To bring within the reach and privilege of the youths and maidens of the present day something of the knowledge and use of this great source of power, this is surely one of the serious obligations of the science teacher of the modern public school.